Psychol Res (1990) 52: 207 – 215

Psychological Research

Direct parameter specification and the concept of perception

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Summary. Stimuli that reach the sensory surface may result in perception, or serve to guide action. How are these two potential consequences of sensory stimulation related? I discuss three aspects of this problem. The *conceptual* aspect concerns the status of the concept of perception in an objective psychology. The *methodological* aspect pertains to the problem of how psychophysics is related to the assessment of performance measures. The *functional* aspect relates to the function(s) of perception for action control. I argue that (a) conceptually, the term perception belongs to a different level of description than the constructs of information-processing models; (b) methodologically, psychophysical judgements and performance measures are not necessarily converging operations; (c) functionally, sensory information can be used for the control of action without perception as a mediating stage (direct parameter specification). Taken together, this suggests that perception should be conceptualized not as a processing stage, but as a class of actions that serve to establish and update an internal representation of the enviroment.

When stimuli impinge on the sensory surface (when there is an uptake of information by the processing system), there are two types of potential consequence. First, the information may be used to control behavior: The organism responds to the stimulus, the information triggers and/or guides the execution of an action. Second, perception may be the result. This paper is concerned with the question of how these two consequences of stimulation are related. I shall discuss what I consider to be three facets of the problem, at different levels of inquiry. Some of the relevant issues have been discussed previously in different contexts (Neumann, 1982, 1987a, 1989, 1990; Neumann & Müsseler, 1990ab; Neumann & Prinz, 1987). In the present paper, I shall focus on commonalities and relationships between them.

Aspects of the problem

The first aspect of the problem is *conceptual*. It concerns the language of psychology. Responding to the stimulus and executing an action are observable events in the physical world and can be described in a physicalist language. In contrast, perception is usually conceived as a private event in the phenomenal world. Hence the question of how the two kinds of consequence of stimulation are related is, in one of its aspects, the problem of how (or, under a more radical perspective, whether) these different kinds of event can be described and conceptualized in a common language. This problem has often been formulated as the question of how intentional states can be incorporated into a naturalistic, ontologically monistic psychology (e.g., Bieri, 1990; Eimer, 1990; Fodor, 1987).

Second, there is a problem of *methodology*. From the beginnings of experimental psychology, there have been two basic methodological traditions in the investigation of mental processes, both of them already described by Wundt (1882): psychophysics, which has aimed at the measurement of the contents of subjective experience, and performance measurement, which has tried to infer mental processes from objective data such as reaction times. Although objective performance measurement has predominated since the advent of behaviorism, and has been promulgated by the information-processing approach (see van der Heijden & Stebbins, 1990), psychophysics has continued to be an important methodological alternative, especially in areas such as sensory processes. There are many phenomena that have been investigated by both kinds of method. This raises the question of how the two methodologies are related and, more specifically, to what degree the "judgements" of psychophysics and the "responses" that we measure in reaction time and percent-correct experiments are converging operations that operationalize the same internal event.

The third aspect of the problem is *functional*. Producing perception and serving to control action are two "vicissitudes" of input information, to use Neisser's (1967) expression. How are they functionally related? For example, is perception an obligatory mediating stage between information uptake and action control? Or are action control and perception independent – perhaps even functionally alternative – consequences of the uptake of information by an organism? The latter possibility may appear to be counterintuitive, but we shall see that it is not without empirical support.

These three aspects of the general problem – the conceptual, the methodological, and the functional aspects – are situated at different levels of inquiry. The first belongs to theoretical psychology, the second is a matter of comparing different methods, and the third concerns the functional organization of the human processing system. But they are certainly not unrelated. In particular, as I shall try to show, a viable answer to the conceptual problem has to take into account possible functional relationships between perception and action control, which may in turn be illuminated by a comparison of results from different methodologies.

The conceptual aspect: Perception vs. information processing

Modern information-processing research appears to have had no particular difficulties with the concept of perception. The prevailing view of perception can perhaps be best understood if we briefly consider its historical development. The information-processing approach to perception has been shaped by three main influences, among others.

There was first the pull of Applied Psychology, which had a strong impact on the information-processing approach, especially in its first decades (see, e.g., Neumann, 1985; Sanders, 1971; van der Heijden & Stebbins, 1990; Scherer, 1988). This resulted in an emphasis on the performance aspect of perception. The question was, at least initially, not so much what we perceive as how well we perceive. Hence there was a strong interest in theories and methods that permit the quantitative measurement of perceptual performance, e.g., signal-detection theory (Swets, 1964) and information theory (Shannon & Weaver, 1949).

This trend was combined with a development in basic research. Although the theoretical systems of neobehaviorism were abandoned by the information-processing approach, the methodological legacy of the behaviorist period was largely preserved in the form of methodological behaviorism, or operationism (e.g., Garner, Hake & Eriksen, 1956). The result was an (often implicit) definition of the area of perception that stressed perceptual mechanisms rather than perceptual experience, and that tried to deduce these mechanisms from performance measures (especially reaction time and percent correct).

Third, it should not be forgotten that, although information-processing psychology was a new approach, it was a new approach to old problems, many of them dating back to nineteenth-century psychology (see, e.g., Neumann & Prinz, 1990; Scheerer, 1988; van der Heijden & Stebbins, 1990). This is evidenced, for example, by a comprehensive collection of papers that appeared in 1969 under the title *Information processing approaches to visual perception* (Haber, 1969). Although the editor introduced the information-processing approach as "one of the newest areas in experimental psychology" (Haber, 1969, p. 1), the majority of the contributions were on classical subjects such as visual masking, temporal-order judgements, reaction time, microgenesis, and attention.

Thus the perception research that emerged within the information-processing approach was both old and new. It was old as far as most of the research topics were concerned. But the (often implicit) concept of perception was new. The analysis of concious contents, or phenomenal experience, was replaced by the assessment of the limits of the system's performance and the modelling of the processing structures that produce these performance limits. The psychology of perception turned into a psychology of perceptional mechanisms and perceptual processing operations. In a way, the study of perception in the information-processing context continued the structuralist, the functionalist, and – to some degree – the Gestalt tradition, but couched in the "black box" framework of neobehaviorism.

As one consequence of this theoretical development, the question that we are concerned with in this paper almost disappeared as a problem. It could disappear because it seemed to have found an exceedingly simple answer, which can be paraphrased as follows. The subject matter of an information-processing analysis is the structures and processes that intervene between information uptake and motor output. Part of these processes, such as feature extraction, short-term visual storage, color encoding, stereopsis, and the like, are occupied with the analysis of stimulus information. Perception is simply a generic term for these processing operations. If we ask how perception relates to the specification of action parameters by input information, the trivial answer is that the former is a component of the latter.

There are two reasons why I find this answer unsatisfactory. One is conceptual, the other empirical.

The conceptual difficulty is that acquiring and analyzing information from the environment is simply not what we mean by "perception". Not each and every uptake and processing of information by an organism is perception. Hence the information-processing point of view amounts less to a reinterpretation of the concept of perception than to its utter abolishment.

To illustrate this argument, consider a very elementary example: a subsystem that subserves respiratory regulation. If the oxygen concentration in the blood decreases beyond a critical level, then chemoreceptors in the glomus caroticum are stimulated. The result is an increased impulse frequency in neuronal pathways that lead to several centers in the medulla oblongata, including an inspiratory and an expiratory center as well as a superordinate pneumotactic center. This produces an increase in the frequency of impulses to the muscles that subserve respiration.

Thus information from the blood-oxygen receptors is used to control the action of muscles, but it would be strange to say that perception is involved in the process. Suppose someone insisted on using this term. Would it then be correct to say that the afferent nerve fiber perceives the receptor potential? Or do the inspiratory and expiratory centers perceive the impulse frequency of the afferent nerve or the oxygen concentration in the blood? Or is it the higher-order pneumotactic center that does the perceiving?

Clearly it does not make sense to use the term perception in this connection. One can end up with as many or as few perceivers as one likes. Components of the system receive information from other components and deliver information to subsequent components. But it seems obvious that this is not what we have in mind when we use the term perception.

The point of this example is not that the information is generated within the organism rather than coming from the environment. Respiratory rate depends not only on blood-oxygen concentration, but, for example, also on the temperature in the environment. The adjustment of the respiratory rate to the output of cold receptors in the skin does not require perception any more than its adjustment to blood-oxygen content. Nor does the example depend on the circumstance that it is a vegetative function that is controlled by input information. In the same way – in terms of information being transferred from one component to the other and being used to control some peripheral event – we can just as well describe, say, the adjustment of bodily posture to the direction of gravity, or the adjustment of walking or running movements to the characteristics of the terrain.

Research contributed by ecological realism has produced many beautiful examples of this kind of adjustment – from the performance of ski jumpers to the diving behavior of the Edinburgh sea gull (for a summary see, e.g., Lee & Young, 1986). When, say, the optical parameter tau (the inverse of the rate of dilation of the retinal image) controls the time at which the sea gull opens its wings or when the ski jumper starts to jump, the functional situation is quite similar to our respiratory-regulation example. There is some input parameter that is being used to control some output parameter. The motor apparatus does not perceive the parameter tau any more than the respiratory center perceives blood-oxygen concentration.

There is a simple lesson to be learned from these examples. Conceptually, perception is not just one stage or one group of subprocesses in the process of using input information to specify output parameters. Perception, as we normally use the term, requires that there be a perceiver. It is the animal or the person that perceives, not some component within the processing system. The term perception in its ordinary meaning is inappropriate if we refer to the processing system and its operations.

There are three obvious possible reactions to this state of affairs. The first is not to care about the usual meaning of "perception" and simply to decree that there is nothing wrong with statements such as "The motoneuron perceives the input from the retina" or "The speed of the approaching object is perceived by the motor control system." According to this proposition, the term perception is synonymous with receiving information. Since we are, of course, free to define our scientific terms, this is a defensible position.

Alternatively, one may completely discard the term perception as a concept taken from folk psychology, which we do not need for a scientific analysis. This has essentially been the behaviorist position.

Third, there is the possibility of retaining the term perception in its usual meaning, while taking pains to use it only with reference to the level of description to which it belongs. There are different grain sizes at which we can choose to describe the course of events when information enters the organism. We can describe it at the level of the different structures and processes involved – the brain centers, information-processing stages, neural units, motor movements, and the like. If we choose this level of analysis, then the term perception is inappropriate. We may, however, choose to describe the same course of events using a coarser grain size. If we refer to the whole organism rather than the structural and functional components as the unit of desciption, then it is appropriate to speak of perception. "The animal (or the person) perceives" would in this case simply be a shorthand expression for the sum of the events that can be analyzed seperately at the fine-grain level.

As a conceptual solution to what seems to be a conceptual problem, each of these three suggestions is acceptable. Nevertheless, none of them is satisfactory. The reason is that the problem is not only conceptual. There is not only a disagreement between the concept of perception in ordinary language and the scientific concept of information-processing operations and mechanisms. Rather, it seems that the conceptual incongruence is at least in part paralleled by empirical discrepancies. As we have already mentioned, information-processing psychology has continued many research traditions that have originated in classical psychophysics. Thus, results from modern objective measurement in the information-processing framework can be compared with subjective measurements intended to assess perception in the ordinary sense of the word.

The results from these different methodologies do not always agree. These intriguing, and possibly revealing, dissociations are likely to be overlooked if we solve the problem at a purely conceptual level, by simply redefining the term perception or by omitting it altogether from our scientific vocabulary. In the next section I shall take a closer look at this aspect of the problem.

The methodological aspect: Psychophysics vs. performance measurement

In spite of the dominance of the information-processing approach, psychophysical methods have not disappeared from modern experimental psychology. They have, for example, continued to play an important role in research fields that are close to sensory physiology. Some of these subject matters have also been investigated with the objective methods of the information-processing approach. There are many cases of psychophysical measurements yielding data that are at variance with what would be expected on the basis of these objective performance measurements. In what follows I shall summarize some of these data. For more comprehensive discussions see Neumann (1989), Neumann and Müsseler (1990ab), and Neumann and Prinz (1987).

One instructive example is visual backward masking. It was one of the main fields of investigation in the first decades of the information-processing approach, and it is also one of the phenomena with which classical psychophysics had begun a century earlier. As a result, there have been two different research traditions in the area. Despite the preponderance of information-processing research, with its objective methods, the subjective research tradition has continued to flourish as well. I shall therefore use this phenomenon to illustrate the difference between the two approaches and the ensuing empirical dissociations.

The first author to describe visual backward masking appears to have been Exner (1868). Working in the laboratory of Helmholtz, Exner already had a high-precision tachistoscope at his disposal in which he was able to present the following stimulus sequence: a lighted semidisk, a lighted complete disk at the same location, and finally a dark interval. Under these conditions the semidisk became invisible if it was exposed for a short time (typically between 15 and 20 ms) and the full disk was presented for a considerably longer period. Exner investigated this phenomenon carefully, using the ascending and descending method of limits to determine the threshold for the disappearance and reappearance of the semidisk. One of his discoveries was that the duration of the second stimulus required for a full masking of the first stimulus varies regularly as a function of the duration of the latter's exposure, a relationship that has been fully confirmed by modern research (e.g., Turvey, 1973).

Exner's (1868) work marks the beginning of a research tradition that has continued until the present. In our current context the methodological characteristics of his psychophysical line of research are of particular interest. The aim of the experiment is not to assess performance, but to measure sensations. Consequently, the subjects' task is not to perform as well as they can in terms of accuracy or speed, but to judge carefully what they perceive. This requires a considerable amount of training. Hence experiments in this research tradition have typically used a small number of experienced subjects, often including the experimenter(s).

Visual backward masking was rediscovered almost a century later by experimenters who had a completely different concept of scientific experimentation in psycholoy. Both Sperling (1960) and Averbach and Coriell (1961) were interested in the processing of briefly presented stimulus arrays. Consider the study by Averbach and Coriell (1961). Their stimulus display consisted of two rows of 8 letters each. The subject's task was to report one of the letters, designated by a bar marker or a circle indicator (a circle that surrounded the letter to be marked). One result was that the circle indicator (but not the bar marker) reduced performance if it was presented with some delay (typically 100 ms). This was a rediscovery of metacontrast, a variant of visual backward masking first described by Stigler (1910).

Averbach and Coriell's name for this phenomenon was not visual masking or metacontrast, but *erasure*. This invention of a new name was altogether appropriate, for although the basic effect was the same, the research context was quite different. Averbach and Coriell's theoretical ideas revolved around stages and mechanism of the information-processing system and, more important in our present context, their subjects had a task that differed substancially from the observer's task in the classical experiments. The task was not to observe anything, and the subjects were not required to make a judgement. As the authors described it, "the subject's task is to name the letter designated by the marker" (Averbach & Coriell, 1961, p. 311). Subjects were encouraged to guess if they were uncertain of the letter's identity. This would have been qualified as a gross "stimulus error" in the classical tradition. But Averbach and Coriell's subjects were to respond to the stimulus, not to judge their sensations; and their task was to achieve the highest possible percent correct score, not to give a valid account of their perceptual experience. What they perceived – which perceptual experience they would have reported, had they been asked – was irrelevant for this task.

Thus the juxtaposition of Exner's (1868) and Averbach and Coriell's (1961) work illustrates the two contrasting approaches. Research into visual masking in Averbach and Coriell's style tries to analyze the processing system by means of assessing task performance. Within this framework, masking consists in a deterioration of performance (e.g., an enhanced error rate). Research in the psychophysical tradition is concerned with sensation and perception. It sets out to measure what subjects perceive, and it does so by asking them to observe and to judge their visual experience. As defined by this approach, visual masking consists in a phenomenal change along some perceptual dimension.

Despite this fundamental difference, psychophysical research and information-processing research have coexisted in the investigation of visual backward masking. As reviews of the field (e.g., Breitmeyer, 1984; Kahneman, 1968; Lefton, 1973; Scheerer, 1973) indicate, it has usually been tacitly assumed that they provide converging operations for the same internal processes. Most theorists have used data from both kinds of method to construct theories of masking.

However, there is evidence that the convergence assumption is not generally true. There are at least two classes of phenomena in the area of visual backward masking in which psychophysical judgements and objective performance measures diverge. One is the Fehrer-Raab effect (Fehrer & Raab, 1962; Neumann & Prinz, 1987). The other is the effect of a distractor on masking (Neumann & Müsseler, 1990ab).

The Fehrer-Raab effect was first reported by Fehrer and Raab (1962) and subsequently confirmed in several studies (e.g., Fehrer & Biederman, 1962; Bernstein, Amundson & Schurman, 1973; for an overview see Neumann, 1982). It consists in what seems to be a dissociation of the subject's psychophysical judgement of metacontrast masking from response latency to the masked target. Even when masking is complete, as assessed by psychopysical judgement, the latency of an a-response (simply pressing a button upon appearance of the stimulus) remains unaffected by the mask.

As such, this effect does not yet conclusively prove a dissociation of response latency from psychopysical judgement. This is because psychophysical judgements in metacontrast experiments refer to dimensions such as presence/absence of the target or its brightness, whereas response latency belongs to the time dimension. Hence the crucial test is a comparison between the response latency to the target-mask sequence and the judged temporal position of the perceived target-mask configuration. When we performed this experiment, the Fehrer-Raab effect was still clearly present, albeit with its size reduced (Neumann, 1982; Neumann & Prinz, 1987). Thus, it appears to be a genuine dissociation of psychopysical judgement from an objective performance measure, response latency.

The second example concerns a dissociation of psychophysical judgement from the other commonly used objective performance measure, the percentage of correct responses. In several studies, summarized in Neumann (1987b) and Neumann and Müsseler (1990a), we have found that a distractor that appears in the visual field affects the metacontrast masking function, enhancing masking in a specific range of stimulus-onset asynchronies (roughly between 60 and 120 ms). This was obtained with psychophysical measurement, either by the method of constant stimuli or by a variant of the signal-detection method in which the subject scales subjective confidence. By contrast, when we used instead percent correct measures in forced-choice experiments, this effect disappeared (Neumann & Müsseler, 1990b). Thus the distractor affects what is perceived and reported in psychophysical judgement, while it does not seem to affect which response the subjects select if they are forced to make a choice.

So it seems that psychophysics and objective performance measurement can be at variance. Masking affects subjective timing differently than it affects response latency. At the level of perceptual experience there is a distractor effect that seems to be absent from forced-choice responding.

The usual reaction of researchers to this kind of situation is the suspicion that there is a flaw in one of the seemingly contradictory sets of data. For example, it might be argued that the psychophysical judgements in these experiments were contaminated by some uncontrolled factor such as a criterion effect. Alternatively, one might think of some artifact in the objective performance measures, e.g., a sophisticated guessing strategy in forced-choice experiments.

This is a possibility, although there is no indication that such artifacts explain the data that I have mentioned. But the suspicion as such is interesting. Why are we inclined to believe that there is probably an artifact if such dissociations occur in the data? It seems that there is a generally accepted tacit assumption about the functional architecture of the system, which predicts that the two measures should converge. This assumption is easy to identify: it postulates that the psychophysical judgement has an internal basis identical to the process or processes that determine objective performance. Since psychophysical judgements require subjects to report their phenomenal experience, the implication is that objective performance measures in visual-masking experiments are likewise based on phenomenal experience. Without this assumption there would be no a priori reason to expect that the two kinds of measures produce similar results.

Thus there is a functional question behind the methodological problem. The usual notion that psychophysical measures and objective performance measures are converging operations is only tenable on the assumption that perception in the ordinary sense of the word corresponds to a stage or a group of subprocesses that form an obligatory step in the information-processing sequence from stimulus to response.

The functional aspect: Mental representation vs. direct parameter specification

The assumption that we must first perceive the stimulus before we can react to it seems natural within folk psychology. It is part of what Ryle (1949) has termed the official doctrine. Within scientific psychology, however, there has been a long tradition of questioning this assumption, which dates back to a century ago. In 1888 Ludwig Lange published a study from Wundt's laboratory in which he introduced the distinction between muscular and sensorial reaction time (Lange, 1888). A year later the first volume of Hugo Münsterberg's *Contributions to experimental psychology* (Münsterberg, 1889) appeared, offering a radical alternative to the official doctrine.

Lange's experiments were simple reaction-time experiments in which subjects had to press a key in response to an auditory stimulus. Lange was interested in the effect of set (*Einstellung*). Subjects could either take a sensorial set, i.e., attend to the stimulus that was to appear, or take a muscular set, i.e., attend to the motor response that was to be executed. Reaction times were different in the two cases. With a muscular set, they were typically around 130 ms. With a sensorial set, they were much longer, typically in the order of 220 ms. As Wundt noted later, the reaction-time distributions are also different in the two cases: The standard deviation is smaller with a muscular set and the distribution is more skewed than with a sensorial set.

Moreover, introspection indicated a qualitative difference between the two cases. Subjects reported that under sensorial set the stimulus was first consciously perceived ("apperceived" in Wundt's terminology) before there was a voluntary impulse to react. By contrast, with a muscular set, there was the experience of reacting before having consciously perceived the stimulus. After training, even the voluntary impulse to react was sometimes reported to be absent. From this Lange and Wundt concluded that there are two ways in which a sensory stimulus can trigger a motor response. Normally, responding requires that the stimulus be apperceived and that there is a conscious decision to react. This is what Wundt termed the complete reaction. With simple, well-practiced actions there is, however, the alternative possibility that the stimulus triggers the motor response directly in a kind of short circuit. This is what Wundt called the shortened reaction.

Wundt's student Hugo Münsterberg was much more radical in rejecting the official doctrine. Arguing from a variety of experiments that anticipated many paradigms of modern cognitive research, Münsterberg suggested that not only do shortened reactions occur with certain well-practiced responses, but they are the rule even when complex processes intervene between stimulus and response. Münsterberg's position is perhaps best captured in this citation: "When we apperceive the stimulus, we have as a rule already started responding to it. Our motor apparatus does not wait for our conscious awareness, but does restlessly its duty, and our consciousness watches it and has no right to give it orders" (Münsterberg, 1889, p. 173: my translation).

Thus, according to this view, conscious perception is not a necessary link in the chain that leads from the uptake of stimulus information to the control of action. This would, of course, readily explain dissociations of performance measures from psychophysical data. If response selection does not depend on perceptual experience, there is no reason to expect that the two kinds of measure will necessarily correlate. In the rest of this paper I shall explore Münsterberg's hypothesis. I shall first cite some evidence in its favor and then discuss some of its problems and implications.

First, two definitions, following Neumann (1989). *Direct parameter specification* refers to the hypothetical case that input information specifies action parameters without (or at least before) giving rise to a corresponding mental representation as a necessary prerequisite. (In our present context, the term mental representation may be equated with perception in the ordinary meaning of the term.) This is the assumed state of affairs that Münsterberg describes in the citation given above. One possible indicator of direct parameter specification is a *dissociation effect*. A dissociation effect exists if a given stimulus specifies action parameters in a way that is not fully compatible with the mental representation of this stimulus. One example is the dissociation of the effect of a distractor on perceptual judgement from that on forced-choice responding, described earlier.

Note that direct parameter specification is a theoretical construct, whereas dissociation is an empirical effect that is intended to operationalize it. The relationship between the two concepts is not logically symmetrical. The existence of a dissociation effect is evidence for direct parameter specification, but if there is direct parameter specification, this need not necessarily result in a dissociation effect. It may well be the case that the way in which a stimulus specifies action parameters is fully compatible with its mental representation, and yet the latter is not a necessary prerequisite for the former. They may be compatible for the simple and obvious reason that both depend upon the same objective situation in the world.

Up to now there has been no systematic experimental literature on dissociation effects. However, a recent search through reports from various areas (Neumann, 1989) has revealed many alleged effects of this kind, although a number of these (often cursory) observations need to be replicated. For a more detailed treatment of what I summarize in the following paragraphs the reader is referred to Neumann (1989).

Some instances are quite spectacular. There is, for example, an observation made by Ivo Kohler who carried out the well-known prism-lens experiments in which subjects wore spectacles or other devices that produce a systematic distortion of the visual world (Kohler, 1951). When subjects wore the lenses for a prolonged period, both motor and perceptual adaptation occurred. However, motor errors disappeared much faster than the distortions of the visual world. In one of Kohler's experiments the subject wore the prism lens for 10 days. Even after this period there remained some visual distortions. However, grasping errors had already disappeared after the first day, and after 6 days the subject was able to participate in a ski excursion in the course of which he had to help rescue the victim of an accident! He still did not perceive objects correctly, but he had no problems in handling them correctly.

Another spectacular finding has recently been reported by Sergent (1987), who performed experiments with split-brain patients. Unlike most previous experimenters, she examined tasks in which information from both visual hemifields had to be combined in order for a correct response to be obtained. For example, her subjects had to perform a lexical-decision task (to decide whether a word or a nonword had been shown by pressing an appropriate button). The stimuli were presented across fixation, i.e., with some letters projecting into the left and some into the right hemisphere. As was to be expected, when asked what they saw, the subjects were only able to report verbally the letters projected to the left hemisphere. However, their motor responses in the lexical-decision task were based on the whole sequence of letters. For example, if the whole sequence formed a word, they correctly pressed the "yes" button in most cases. Similarly, they were able to perform mental arithmetic on two digits presented to different hemispheres even though they were unable to name both of them. Sergent found similar dissociations in other tasks and summarized her findings by concluding that there was "coexistence of perceptual disunity and behavioral unity" (Sergent, 1987, p. 1375).

These are intriguing examples. They are not without methodological problems, however; for example, small numbers of subjects. In this respect, they are like what is probably the best-known spectacular example, the blindsight phenomenon. As reported by Weiskrantz, Warrington, Sanders, and Marshall (1974), patients with a scotoma in the primary visual area are surprisingly good when they have to point to a target or make a saccade at it, even though they report that they cannot see it. As was pointed out by Campion, Latto, and Smith (1983), there are many methodological problems (e.g., residual vision, stray light) associated with this effect. Taken in isolation, these spectacular examples should therefore be interpreted with great caution. However, there are many less spectacular examples of dissociations in the non-clinical experimental literature. They can be found in studies that were often directed at a different topic and that produced the dissociation as a side effect.

As has been argued elsewhere (Neumann, 1989), these cases can be subdivided in different ways. First, there are direct and indirect indicators of an dissociation. The examples that we have discussed so far are instances of direct indicators, since they allow a direct comparison of psychophysical data and objective performance measures. Indirect indicators of an dissociation can be found in experiments in which different performance measures were determined and showed divergent effects of the same sensory parameter. For example, it is commonly believed that stimulus intensity affects reaction time by influencing perceptual latency. If this is the latency of a mental representation, the size of this effect should be independent of the kind of output measure. However, Hughes and Kelsey (1984) found that stimulus intensity has a much smaller effect on saccade latency than on the latency of a manual response. This indicates that at least one of these two effects – and possibly both – is not fully mediated by the latency of a mental representation.

A different subdivision can be based on the nature of the dimension that exhibits the dissociation. The dissociation may, first, concern the detection of the stimulus. The Fehrer-Raab effect, described earlier, is a case in point. Second, the dissociation may be spatial. For example, Bridgeman, Kirch, and Sperling (1981) reported a dissociation of the phenomenal position of a target stimulus that was apparently shifted by means of induced motion from the position at which subjects localized it by manual pointing, which was largely unaffected by the induced motion.

Finally, there is a broad class of dissociation effects in which the dissociation is temporal. For example, Rutschman and Link (1964) compared reaction times and temporal-order judgements to visual and auditory stimuli. They found that the auditory stimuli were perceived as appearing after the visual stimuli; yet response latency was much faster to the former than to the latter. Similary, Barr (1983) discovered that the spatial frequency of visual stimuli does not influence temporal-order judgement, although it has a strong and reliable effect on response latency.

Both of these effects have recently been successfully replicated in our laboratory under carefully controlled conditions (Neumann, Koch, & Tappe, unpublished data). We may therefore assume that at least some of the published dissociation effects are genuine. Direct parameter specification is likely to exist.

While this seems to be a safe conclusion, we are still very much in the dark as to the scope and functional significance of direct parameter specification. Wundt's notion that it is restricted to certain highly practiced stimulus-response pairings is still a possibility, as is Münsterberg's position that it is ubiquitous, and that conscious perception is not at all functional for the sensory control of action. As is discussed in Neumann (1989), there are certain pieces of evidence that point more to Münsterberg's than to Wundt's position, though there are, of course, options in between. In any event, the matter is far from settled. We badly need more empirical data on the range of dissociation effects.

Conclusion: Direct parameter specification and the concept of perception

Whatever we learn in the future, the very existence of direct parameter specification has an important implication for the concept of perception. As was stated at the end of the first section, there are three obvious and logically flawness conceptual solutions to the problem of perception as a theoretical concept: to omit it altogether; to identify it with the operation of receiving information; or to use it as a term for the ensemble of processing operations, described at the level of the animal or the person. It is now easy to see why none of these solutions is satisfactory. They all obscure the distinction between perception and direct parameter specification. We cannot dismiss the term perception, since it designates a specific class of empirical phenomena that can be assessed by psychophysical methods. We cannot equate perception with receiving information from some other stage of processing, since this would also include direct parameter specification and hence annihilate the distinction. We cannot use the term to designate the level of description that refers to the whole animal or person, because direct parameter specification can equally well be described at this level.

Thus the existence of direct parameter specification puts us in a dilemma. The conceptually sound solutions that we have so far considered lead to a concept of perception that is empirically empty; while, as we have seen in the first section, simply blending the concept of perception into an information-flow model is conceptually untenable.

This is not only true for the suggestion that perception should simply be conceptualized as a generic term for feature extraction, pattern analysis, stereopsis, etc. It also holds for a second variant of the same idea. Once one has accepted that perception cannot simply be equated with all kinds of processing and use of sensory information, it is tempting to equate it instead with the activity at some particular stage within the functional architecture of the processing system – e.g., Wundt's (1903) apperception; the SVE (subjective visual experience) box in Mayzner and Tresselt's (1970) information-processing model; or Marcel's (1983) "recovery." But this still disregards the circumstance that it is the animal or the person that perceives, not a component of the system.

One might argue that this is merely a matter of wording. Couldn't the problem be solved by simply saying that the statement "The person perceives" is equivalent to "There is activity in component x (the apperception, SVE, recovery component) of the system"? There are at least three reasons why this is not a satisfactory solution.

First, this suggestion does not explain why it would be appropriate to equate the activity of this particular component of the processing system, but not that of other components, with a characteristic that can be ascribed to the person as a whole. For example, why shouldn't it be equally appropriate to translate the activity of the feature-extraction component into the statement "The person extracts features"? Or, to return to our original example, what would then be wrong with ascribing the registration of blood-oxygen content to the person as a whole? Clearly this suggestion begs the question by simply defining away the problem of how to conceptualize perception.

Second, the suggestion neglects the empirical fact that the activity of the supposed component x is at best a necessary, but not a sufficient condition for the occurence of what we mean by perception. When a person or an animal perceives, more is going on than what might be equated with "apperception" or "recovery." As Gibson (e.g., 1966, 1979) has pointed out, perception includes peripheral as well as central events, and it encompasses motor as well as sensory processes. (For a recent review of the contribution of motor processes to perception see Bridgeman, 1990.)

Third, there is a problem that I have so far deliberately circumvented, namely intentionality. Perception is intentional[1] in the sense that to perceive inevitably implies perceiving something. To perceive is a relational term. It requires not only a perceiving subject, but also an object of perception. Equating perception with the activity of a component x of the system leaves this aspect of perception completely unexplained.

Taken together, these difficulties suggest that a perspective on perception not unlike that of Gibson and his followers (e.g., Turvey, Shaw, Reed, & Mace, 1981) may be more appropriate than the information-processing perspective. According to the Gibsonian view, perception is an activity of the whole animal or person, including peripheral motor activities. Further, Gibson's theory encompasses an "ecological concept of intentionality" (Turvey et al., 1981, p. 242), namely the assertion that perception is the (direct) perception of (real) objects.

Thus the Gibsonian concept of perception avoids the conceptual problems that render an account of perception in terms of a specific component of the information-processing system unattractive. The problem with the Gibsonian perspective is that it does not easily offer a basis for the distinction between perception and direct parameter specification. On the contrary, direct parameter specification in the present sense of the term (not to be confused with Gibson's direct perception) is explicitly included under the term perception (e.g., Michaels & Carello, 1981, p. 1; Turvey et al., 1981, pp. 240 f.)

What we seem to need, then, is a concept of perception that preserves the virtues of Gibson's approach, but accounts for the empirical differences between perception and direct parameter specification. One possible solution (for details see Neumann, 1990) is to conceptualize perception not as any activity of picking up information for the control of action, but as a specific kind of information pickup, which serves to establish and update an internal representation of the environment. Orienting towards novel stimuli and exploring the environment belong to this category.

There is some ground for the speculation that these interrelated functions – the use of acquired information to establish an internal representation, and exploration-type activity – are specific to higher vertebrates (see Neumann, 1990). Further, orienting and exploration normally engage the whole organism and are therefore poorly compatible with alternative actions. This leads to the conjecture that direct parameter specification is a normal, perhaps *the* normal, mode of using information from the environment. Perception may be a very special function with specific characteristics (e.g., a surprisingly long latency, see Neumann, 1987a). But at present this is mere speculation. As was pointed out earlier, the distinction between perception and direct parameter specification needs to be further explored empirically before we can draw safe conclusions.

[1] This usage of the term intentional, which dates back to Brentano and is common in the philosophy of mind, is different from its meaning in theories of information processing, where intentionality usually means that a process is willed rather than automatic.

Acknowledgements. Preparation of this paper was supported by grant Ne 366/1 – 1 from the Deutsche Forschungsgemeinschaft (DFG). The paper is partially based on talks given at the University of Leiden (NL) in April 1989 and at the University of Osnabrück (FRG) in January 1990.

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